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Steel Shot

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July 9, 1982

TO WHOM IT MAY CONCERN.

Attached hereto is a copy of the final report on the Lacassine lead/steel shot shooting test.

Inasmuch as the results of this test differ considerably from some previous tests, and the entire subject is a highly volatile one, it is prudent to recognize that the current study has been the subject of substantial criticism. Attached to this report is a copy of a highlighted letter which details the criticism received.

Although we do not attach any substantial significance to these criticisms, we are making them available to all recipients of the report for their consideration.

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March 5, 1981

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FILE NO

John P. Rogers, Chief
Office of Migratory Bird Management
U. S. Fish and Wildlife Service
Department of the Interior
Washington, D. C. 20240

Dear Mr. Rogers:

Thank you very much for your letter of February 19, 1981. ~~Before addressing the substance of your inquiry, I am~~ constrained to mention our disappointment regarding the meeting between Messrs. Newsom, Soileau and Smith which, from the dates, had to have been held during the waterfowl symposium in New Orleans. Our committee member, Mr. Frank G. Harris, III, not only attended the entire symposium, but on one or more occasions spoke with each of those gentlemen. ~~Electing not to invite the attendance of Mr. Harris, despite~~ certain knowledge of his presence, further evidences the conscious exclusion of the participation of our committee. Once again we have been told of meetings and formulation of policies after the fact. Given the history of such behavior throughout this project, we must note again the impression that we are being used as a token of participation by the private and Congressional sectors. The continuation of such intentional exclusions of our committee members at the formative level actively prevents the cooperation mandated by Senator Johnston and former Secretary Andrus for the Louisiana plumbism project.

Like the other members of the liaison committee, I was pleased to learn that the mortality survey was conducted successfully. We await with interest the detailed results of the necropsy work, as well as the summary.

We were disappointed to learn that the shooting test failed for inadequate bag results--~~but we were not surprised~~ that such was the case. We know that those persons responsible for conducting the test worked under a considerable

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timing handicap because the "go" decision for the shooting test came so late in the year. We also know that Mr. Newsom at the LSU project, along with the many others involved in the scientific and personnel training facets of the test, did an outstanding job, particularly in light of the time pressures under which they were working. ~~We feel, however, that failure of the test resulted in large part from the inappropriateness of the hunting blinds from the standpoints of construction and location, and from the logistical difficulties imposed on potential participants. Blinds were located quite close together, and were spaced at regular intervals, along an arbitrarily selected flank line, rather than being scattered over a larger geographical area at locations selected on the basis of pond attractiveness or known flyways. As described to me, the "Maginot line" of blinds sounds like a typical site plan for a Midwest or Far West "preserve" situation, in which blinds are spaced as closely together as feasible along the perimeter fence of a preserve, depending on random pass shooting, not "decoy" shooting in a marsh. It did not sound like the typical Louisiana marsh setup.~~

~~More importantly, the individual blinds as described to me were not constructed in keeping with tried and true Southwest Louisiana marsh blind design. Rather than installing the preferred, low silhouette, partially-buried tank or box blind, with low profile boat hide, or even the less satisfactory but acceptable low-silhouette, minimum "platform," they were large, obtrusive platforms resembling North Louisiana lake blinds, with high, square profiles, large boat-hides and house-like silhouettes. Frankly, it is a wonder that the bag results were as good as they were.~~

~~Not only does such blind construction and location diminish the probability of collecting a sufficiently large bag to be statistically valid, it also contributes to bias in the results. First, by using closely spaced, arbitrarily aligned and non-typically designed blinds, the setup was not a valid imitation of actual hunting conditions in the geographical area of Louisiana in which the test was conducted. More importantly, it imparts a "no difference" bias to the test. "Big duck" shooting opportunities occur for the most~~

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part from and after sun-up when the larger birds are normally expected to move. In the 1980 test setup, the late-flying big ducks were quite likely "scared off," in part by ambient shooting at other blinds so close together, but even more so by the large, unnatural blinds themselves, all the more visible, individually and collectively, after sunrise. On the other hand, the early, "daybreak" flyers (predominantly greenwinged teal) usually traverse the shooting area during the half hour before sunrise, flying low and close, with little or no "blind shy" avoidance in the half-light of dawn, even as to the monolithic blinds built for this test. That situation skews the test by producing an inordinately high percentage of close-in shots on small birds which die quite easily. Consequently, the test last season may well have been "loaded," albeit quite by inadvertence, to reflect little or no ballistic difference between lead and steel shot. Such an abnormally high percentage of low, close range shots at small, easy-kill birds could be a seriously distorting factor.

The logistics of the hunting would also have been a discouraging factor to all but the most experienced Lacassine hunters. To participate in the test, a pair of hunters had to bring their own motorboat, pirogue and decoys to the headquarters of the preserve (in its northerly area) by 4:00 a.m., hours before daylight. The boat and motor had to be hunter-provided no matter what the "draw," but additionally, each hunting pair needed to bring decoys, steel shot shells and pirogue because the testers only supplied shells, pirogue and decoys for hunters who drew one of the twenty "observer" blinds; a draw of one of the ten "standby" blinds would require the hunters to provide those items of their own.

More importantly, the hunters were not escorted to the hunting area but had to navigate by boat, alone in the total darkness of night (at times under the added hazard of fog or inclement weather) with nothing but a flashlight and map, all the way from the launch site, to and along the hazardous Intracoastal Canal, to the general area of the hunt. Anyone not thoroughly familiar with that particular geographic area of Lacassine and the Intracoastal Canal would be foolhardy to attempt such a trip at night or in foul weather, unaccompanied by guide or preserve personnel.

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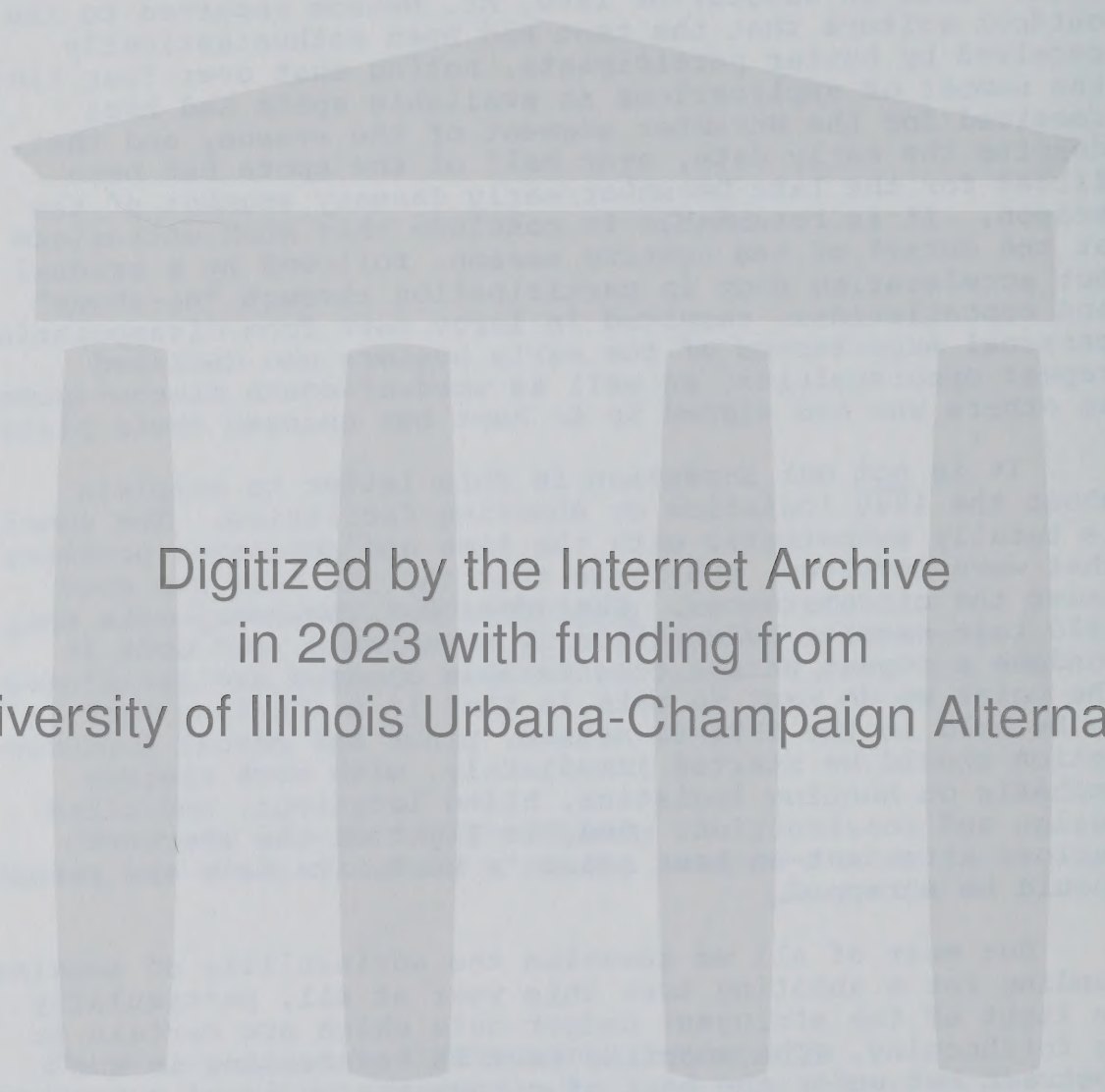
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We feel the foregoing is further confirmed by the rapid fall off in hunter interest during the course of the shooting test. Late in October of 1980, Mr. Newsom reported to the outdoor writers that the test had been enthusiastically received by hunter participants, noting that over four times the number of applications as available spots had been received for the November segment of the season, and that, despite the early date, over half of the spots had been filled for the late December/early January segment of the season. It is reasonable to conclude that such enthusiasm at the outset of the hunting season, followed by a gradual but accelerating drop in participation through "no-shows" and cancellations, resulted in large part from disappointing personal experiences of the early hunters who declined repeat opportunities, as well as word-of-mouth discouragement to others who had signed up to hunt but changed their plans.

It is not our intention in this letter to complain about the 1980 logistics or shooting facilities. The committee is totally sympathetic with the time and personnel problems that were involved, and feels that the job done was good under the circumstances. ~~That does not, however, make the 1980 test results scientifically acceptable, nor does it condone a repeat unless considerable changes are instituted.~~ The point we do want to make is that if a shooting test is to be held in the 1981-82 season, plans and actual implementation should be started immediately, with much greater emphasis on hunting logistics, blind locations, and blind design and construction. ~~And, in light of the aberrant factors attendant on last season's test, its data and results should be scrapped.~~

But most of all we question the advisability of seeking funding for a shooting test this year at all, particularly in light of the stringent budget cuts which are certain to be forthcoming. ~~The shooting test is interesting in the abstract but under the best of circumstances is of questionable scientific validity for proving ballistic equivalency of the substitute shot vis-a-vis lead. There are too many variables and too much left to chance in the human factors area. The real value of a shooting test in the field (assuming the results show either "no difference," or reasonable equivalency) would be as a public relations tool to help gain hunter~~



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acceptance of steel shot, if its use is inevitable. Until and unless necropsy and other scientifically dependable tests can locate with reasonable precision, true "hot spots" of plumbism in Louisiana, wherein use of non-lead shot must be mandated, we feel the shooting test should be postponed and the funds for it devoted to higher priority uses, such as acquisitions of breeding and wintering wetlands, preserving existing wetlands, solving the man-made problem of "short-stopping," and researching such waterfowl problems as avian cholera, botulism and, especially, those diseases which result from artificial, man-made overcrowding of waterfowl in state and federal preserves.

Please do not construe our observations and opinions as opposition to conducting another shooting test in Louisiana. We are merely setting out the context and perspectives in which we feel that decisions should be made, first whether funds for shooting tests should be requested for this year, and second, as to implementing better logistics and better blind location and construction should the decision be made to re-conduct the test.

The members of the liaison committee are unanimous in the foregoing observations, opinions and recommendations, and offer them in the continued spirit of positive cooperation which we deem to be so important to the success of the entire project. Each of us deeply appreciates your personal involvement, and the dedication of the state and federal employees who work in it. Kindest personal regards.

Sincerely,


Jacques L. Wiener, Jr.

JLW, Jr.:prm

FINAL REPORT

RELATIVE EFFECTIVENESS OF NO. 4 STEEL AND NO. 6 LEAD SHOT
FOR HUNTING DUCKS--THE LACASSINE STUDY

Cooperative Agreement No. 14-16-0009-80-1014

Submitted to: Louisiana State University
Louisiana Department of Wildlife
and Fisheries
U.S. Fish and Wildlife Service

by

Louisiana Cooperative Wildlife Research Unit

28 June 1982

RELATIVE EFFECTIVENESS OF NO. 4 STEEL AND NO. 6 LEAD SHOT FOR HUNTING
DUCKS--THE LACASSINE STUDY

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Abstract: Relative effectiveness of No. 4 steel and No. 6 lead shot for hunting ducks was tested under field conditions on Lacassine National Wildlife Refuge during the 1980-81 and 1981-82 waterfowl seasons. Federal Hi-Power No. 6 lead was compared with Federal No. 4 steel in 2-3/4 inch 12-gauge loads. Significantly more ducks were hit per shot fired ($P < 0.01$) with No. 6 lead (mean = 0.196) than with No. 4 steel (mean = 0.159). Ducks hit per shot was nearly twice as high for both loads at closer ranges than for shots greater than 32 m (35 yd). The proportion of ducks crippled per hit was significantly higher ($P < 0.01$) for No. 4 steel (mean = 0.334) than for No. 6 lead (mean = 0.236), resulting in a 41.5% increase in cripples per hit with No. 4 steel. The proportion of ducks crippled per hit was significantly greater for both load types at longer ranges (> 32 m).

INTRODUCTION

Controversy continues to surround the use of non-toxic steel shot for hunting waterfowl, even though much research has been conducted on steel shot (Andrews and Longcore 1969, Kozicky and Madson 1973, Nicklaus 1976, Mikula et al. 1977, Anderson and Roetker 1978, Anderson and Sanderson 1979, Humburg et al. 1982). Results of most steel shot studies have shown little or no differences in effectiveness between steel and lead shot. However, some hunters have not accepted the results of these studies, maintaining that there is a real difference in effectiveness when they use steel shot while hunting ducks.

In July 1979, a group of Louisiana hunters petitioned the U.S. Fish and Wildlife Service to conduct further research on steel shot in Louisiana. Subsequently, a shooting study was conducted at Lacassine National Wildlife Refuge by the Louisiana Cooperative Wildlife Research Unit in cooperation with the U.S. Fish and Wildlife Service and the Louisiana Department of Wildlife and Fisheries. The purpose of the Lacassine shooting study was to determine the relative effectiveness, in an actual duck hunting situation, of the most popular lead load used by Louisiana duck hunters and the steel load that was ballistically most comparable of those readily available to hunters.

We are grateful to the 33 observers and more than 1000 hunters who participated in the study. Thanks are due B. Brown, K. Ouchley and the staff of Lacassine National Wildlife Refuge for their valuable help. We thank D. Hewitt, P. Shealy and students of the Louisiana Cooperative Wildlife Research Unit for conducting drawings for hunts. Appreciation

is extended L. Soileau, Louisiana Department of Wildlife and Fisheries, for assistance given through all phases of this study. R. Aycock provided valuable help with technical and financial matters. T. Roster deserves thanks for training the observers and assisting in other ways. We also express our appreciation to D. Hayne and P. Geissler who assisted in study design and review of statistical methods. The project was supervised through the Louisiana Cooperative Wildlife Research Unit; Louisiana State University, U.S. Fish and Wildlife Service, Louisiana Department of Wildlife and Fisheries and Wildlife Management Institute, cooperating.

DESCRIPTION OF STUDY AREA

The study was conducted on Lacassine National Wildlife Refuge (LNWR) located in the coastal Chenier Plain marshes of southwest Louisiana approximately 24 km (15 mi) southwest of Lake Arthur (Fig. 1). The study area is a freshwater marsh dominated by dense stands of bull tongue (Sagittaria lancifolia) and maidencane (Panicum hemitomon) surrounding shallow open water ponds. Aquatics, including water lotus (Nelumbo lutea) and white water lily (Nymphaea odorata), were abundant in open ponds. Ponds with dense aquatic vegetation became more open as temperatures dropped and hunting seasons progressed. Marsh-vegetation was fairly homogeneous throughout the study area; however, pond-size and interspersions of vegetation varied considerably between blind sites.

Water depth in ponds usually varied from 10 cm to 30 cm (4-12 in), dependent upon wind speed and direction, but was generally similar for all blinds. Marsh water levels were lower during 1980 than 1981. Low water levels and strong north winds in 1980 sometimes left decoys stranded on the mud, causing poor hunting conditions. Lacassine marsh has a soft bottom which makes walking very difficult in most areas and impossible in several of the areas that were hunted. Thus, hunters were frequently forced to use a flatboat or a dog to retrieve the downed ducks.

Hunting blinds were constructed on pond edges throughout the study area. All blinds were consistently located on the southeast side of a pond with each blind facing towards the northwest. Each blind consisted of a 1.2 m by 2.4 m (4 x 8 ft) platform surrounded by mesh wire. A blind for hiding a small flatboat was attached to the rear. The entire structure was camouflaged with Roseau cane (Phragmites communis),

a common plant in the study area. Traditionally, hunting blinds have been camouflaged with similar vegetation, although blinds used in this study were larger than a typical south Louisiana marsh blind.

The study area has received heavy hunting pressure for many years. Waterfowl hunters have been required to use steel shot on the Refuge since 1974.

Historically, a variety of duck species have been taken by hunters using the study area. Mallard (Anas platyrhynchos), mottled duck (Anas fulvigula), pintail (Anas acuta), gadwall (Anas strepera), wigeon (Anas americana), blue-winged teal (Anas discors), green-winged teal (Anas crecca), wood duck (Aix sponsa), shoveler (Anas clypeata), ring-necked duck (Aythya collaris), and lesser scaup (Aythya affinis) make up the majority of the bag at LNWR (Appendix A).

METHODS

The study was conducted during the duck hunting seasons of 1980 and 1981 (November, December, and January). Morning-only hunting was allowed on Wednesday through Sunday for a total of 39 hunting days each year. The test procedure was for a trained observer to occupy a duck blind with hunters. Observers recorded data while participants hunted in their normal duck hunting manner. Observers used the same procedures both years.

Hunters applied by mail to participate for each hunting day and were selected by a drawing of applications. Hunters were preassigned by random numbers to blinds for each day of hunting. For each day of hunting in 1980, hunters were assigned to 19 of 23 blinds used in the study. In 1981, hunters were assigned to 12 test blinds. The number of blinds was reduced because only 12 of the 23 blinds used in 1980 provided sufficient data to be analyzed statistically.

About 20% of the blinds were filled by additional hunters on a first-come daily basis when either 1 or both of the assigned hunters did not show up to hunt. In 1980, such hunters were given their choice of empty blinds; in 1981, they were generally assigned to empty blinds. Another change that occurred between years was that hunters had to provide their own transportation to traverse the 10 to 19 km (6 to 12 mi) of water from the Refuge headquarters to the blinds in 1980; all boat transportation to blinds was provided hunters in 1981.

Observers came from many different states and had a wide range of background and experience. The educational level of observers in 1980 ranged from not completing high school to a Master's degree. The majority had a college background in wildlife. In 1981, all observers

had a college background in wildlife with 3 having a Master's degree. For each day of hunting, observers were randomly preassigned to blinds.

Observers were given 2 weeks of intensive training prior to each hunting season. They were trained to estimate distances both visually and using a mechanical rangefinder (Rangematic Ranging 610) through a series of repetitive exercises and testing of individual ability. Field training included instruction on data gathering as well as distance measurements under actual hunting conditions.

Standard factory loads of 2-3/4-inch 12-gauge Federal Hi-Power No. 6 lead and Federal Steel Shot No. 4 were tested in this study. No. 6 lead was chosen because it was determined to be the most popular load for duck hunting, based on a mail survey of randomly selected Louisiana duck hunters conducted by the Louisiana Department of Wildlife and Fisheries (Appendix B). No. 4 steel was chosen as the steel load for comparison because, of the steel loads readily available for use by duck hunters, it was judged to be ballistically most comparable to the No. 6 lead load. Steel shot shells used were factory loads of 1-1/8 ounce No. 4 steel with 213 pellets with an average muzzle velocity of 1365 fps. Lead shot shells used were a factory load of 1-1/4 ounce No. 6 lead with 279 pellets and average muzzle velocity of 1330 fps. The velocities indicated are according to manufacturers' standards.

Design of the test included a double-blind secrecy on knowledge of load-types that were tested. Observers and hunters did not know what loads were being tested. Also, hunters and observers did not know whether lead or steel was used in their blind each day. To further confuse hunters attempting to determine load type, 4 secondary "confusion" loads were used in the test about 5% of the time.

All markings were removed from the shells, thus making all loads identical in external appearance. Test loads were then coded in special boxes with 26 letters of the alphabet. Eleven letters were assigned to load-type I (later identified as No. 6 lead), 11 were assigned to load-type II (later identified as No. 4 steel), and 1 was assigned to each of 4 "confusion" loads. Identification of the loads being tested was not revealed to the shell handler, project field supervisor, or statistician until the study was completed. For each day of hunting, 1 shell was retained from each blind to provide verification of the shot-type used. Observers distributed 50 shot shells (2 boxes) to each hunter in the blind. Observers were required to account for all shells after each hunt.

One load was assigned to each blind each day using a restricted randomization scheme. First, blinds were separated into groups based on size of ponds in the marsh. Days were then grouped into 4-day blocks in 1980 and 8-day blocks in 1981. Confusion loads were assigned at random, with the restriction that 2 confusion loads were assigned to day-block and blind-group selected. Test loads were assigned randomly to the remaining blinds and days with the restriction that each blind used each load the same number of days and each blind-group used each load equally often on a given day.

Observers recorded only shots fired at ducks. Data were recorded by attempts, defined as 1 or more shots fired at a flight of ducks, usually without reloading. For each attempt, the observer recorded the distance at which the first shot was fired, number of shots fired, number of ducks bagged, number of ducks hit but not recovered, and number of shots fired at wounded ducks on the water. Distance recorded

on an attempt was the observer's estimate of the distance to the closest duck when the first shot was fired. All measurements were recorded in yards but converted to meters for consistency of presentation. A downed duck was not recorded as bagged until it was recovered by the hunter. A cripple was defined as a duck that was visibly hit but not retrieved, including any dead duck not recovered (bagged) by the hunter.

Observers recorded whether or not a retrieving dog was used and choke information on the gun used by each hunter. Observers asked each hunter, "Do you think you know which type of shot shell you were using"? If a hunter answered, "yes", he was then asked whether he thought he was using lead or steel. Also recorded was whether or not the observer had any evidence that the hunter actually knew what shell-type was used. When hunters returned to the check station each day, number and species of ducks bagged were recorded and checked against the observer's data.

For analysis, the experimental unit was defined as the results of all attempts with the same load for a particular blind each season. This unit was then split into distance categories to establish sampling units. Also for analytical purposes, hits were defined as the sum of ducks bagged and ducks crippled. The proportion of ducks hit per shot and crippled per hit were calculated for each sampling unit. These proportions were transformed using the angular transformation ($\text{Arcsine } \sqrt{P}$) (Steel and Torrie 1980). Transformed variables were used in a weighted analysis of variance (ANOVA), weighting hits per shot with the number of shots and crippled per hit with the number of hits. Means were calculated by back-transforming the means of the transformed variables.

The Lacassine study was designed to have sufficient power to detect a 33% or greater difference in ducks crippled per hit between No. 6 lead and No. 4 steel if 2500 or more ducks were hit. A difference of 14% or more in ducks bagged per shot could be detected if at least 10,000 shots were fired (D. Hayne, in a letter to L. Soileau, Louisiana Department of Wildlife and Fisheries dated October 24, 1980) (Appendix C). Because insufficient data were collected in 1980 to meet these criteria, the study was continued for a second year.

Screening the data to identify uncontrollable factors that might be confounded with the effects of the load was accomplished by cross-classification of the data not utilizing the experimental units. Chi-square tests were used to test whether these classification criteria were independent.

RESULTS

Data Selection

Observations were summed for each season for all hunters shooting from the same blind with the same load type. Only those parties shooting No. 6 lead shot or No. 4 steel shot were included in the analysis. Observations on an attempt were deleted when some information--usually distance to first shot or number of cripples--was not recorded. For each year, 12 blinds were available on each of 2 loads yielding 48 experimental units. Data were further divided into 2 distance categories, totaling 96 sampling units in the study (Table 1).

Distance data were grouped into 2 categories after studying the frequency of occurrence of birds bagged, birds crippled and shots fired at 4.6 m (5 yd) intervals (Appendix D). Attempts to separate the observations into 3 distance categories were discontinued when inspection showed several blind-load-distance groups would have zeros in some categories for ducks crippled or bagged. Distance categories used in the analysis were less than or equal to 32 m (35 yd) and greater than 32 m (35 yd) for first shot fired. The 32 m (35 yd) class separated the number of ducks crippled into 2 nearly equal groups, while the 27.4 m (30 yd) class separated the number of ducks bagged and the number of shots fired almost evenly. The 32 m (35 yd) class was chosen because the number of ducks crippled was the variable with least data and considered the variable of greatest importance.

A total of 834 blind-days was recorded when No. 6 lead and No. 4 steel were used. In 1980, 11 of 23 test blinds (109 blind days) did not produce sufficient data to be analyzed (at least 50 birds bagged plus

crippled). Data for the 11 excluded blinds are listed in Appendix E.

Certain uncontrolled factors within the study, such as ability to identify load being used, different chokes of shotguns used, and use or non-use of a retrieving dog could be confounded with the effects of load. Each was addressed separately to identify confounding effects.

Either 1 or both hunters in 39% of the hunting parties attempted to guess which load-type they were using. Nearly 70% of the hunters who guessed thought they were using steel shot. The data did not indicate that hunters who guessed were actually shooting either load more frequently (X^2 test, $P=0.54$). More hunters guessed correctly than would have been expected by chance (X^2 test, $P<0.01$). This could mean that hunters were either basing their guess on a variety of cues that gave them a higher chance of being correct or that some hunters could differentiate between loads being tested.

Because hunters that participated repeatedly had more opportunities to learn to distinguish between loads, records of these individuals were reviewed. Only one frequent participant consistently guessed correctly. Based on this, we think that few hunters who attempted to guess what they were shooting could actually differentiate between the loads being tested.

About 57% of the hunters used modified chokes, 40% used full chokes and 3% used open chokes (Appendix F). No significant difference was found in the number of times each choke-type was used by hunters shooting No. 6 lead and No. 4 steel (X^2 test, $P=0.97$).

The final uncontrolled factor considered was the use of retrieving dogs by some hunting parties. During 1980, 21.8% of the hunting parties used dogs; in 1981, 16.3% of the hunting parties used dogs (Table 4). A significantly higher proportion of the hunters using No. 4 steel used dogs in 1980 (χ^2 test, $P=0.01$), but the proportion of hunters using dogs was nearly identical for the 2 loads in 1981 (χ^2 test, $P=0.84$). Little difference was found in ducks hit per shot fired between hunters with or without dogs, but significantly fewer ducks were crippled per hit for hunters with dogs in both distance categories (binomial test, $P < 0.02$, ≤ 32 m; $P < 0.01$, > 32 m) (Table 5).

Evaluation of the effects of these factors for each load indicated that they were not sufficiently important to require deleting observations or incorporating additional factors in the analysis.

Comparison of Loads

The loads were compared using data from 8023 No. 6 lead shells and 8615 No. 4 steel shells shot at ducks during the 2-year study. A total of 802 ducks were crippled: 366 with lead shot and 436 with steel shot. Hunters bagged 2228 ducks: 1242 with lead shot and 986 with steel shot. Table 6 summarizes the raw data by year and distance.

Mallard and gadwall were the most common species bagged followed by green-winged teal and blue-winged teal (Table 7). The relative abundance of each species in the bag was significantly different between years (χ^2 test, $P < 0.01$) with a higher proportion of green-winged teal and pintail in 1980 and blue-winged teal, wigeon and scaup in 1981. There was no evidence that the species composition of the bag differed between loads (χ^2 test, $P=0.42$, 1980; $P=0.18$, 1981).

The hypothesis that the ducks hit per shot was the same for No. 6 lead and No. 4 steel was tested with the ANOVA shown in Table 8. Blind was considered a random effect, while load and distance were considered fixed effects. Both load and distance were significantly different ($P < 0.01$). None of the interaction terms for the variable were significant. No. 4 steel had an 18.9% lower hits per shot mean (0.159) ^{16.12 hits/shoot lead} than No. 6 lead ^{20.04 hits/shoot lead} (0.196) (Table 9). A plot of ducks hit per shot at 4.6 m (5 yd) intervals showed that No. 6 lead consistently hit a higher proportion of the shots (Sign test, $P < 0.01$) (Fig. 2).

The ANOVA for the variable ducks crippled per hit showed that load and distance again were both significant ($P < 0.01$) (Table 8). The blind by distance interaction was significant ($P = 0.03$) for the variable ducks crippled per hit. The mean number of ducks crippled per hit was 0.236 ^{22.76%} for No. 6 lead ^{30.66%} and 0.334 for No. 4 steel: a 41.5% increase in crippling rate for the steel load over all distances (Table 9). For both loads combined, a 77% increase in ducks crippled per hit resulted for shots beyond 32 m (35 yd) over shots at less than 32 m. A plot of ducks crippled per hit against distance in 4.6 m (5 yd) intervals for the 2 loads shows that the use of No. 6 lead resulted in fewer ducks crippled per hit than No. 4 steel (Sign test, $P = 0.035$) (Fig. 2).

The effectiveness of No. 6 lead and No. 4 steel in killing wounded ducks that fell into the water was compared (Table 2). The difference between loads was within the realm of what would be expected by chance (χ^2 test, $P = 0.12$). Insufficient evidence was found to indicate a difference in the proportion of wounded ducks shot at on the water that were recovered by hunters using No. 6 lead and No. 4 steel.

DISCUSSION

The Lacassine shooting test compared the lead load most often used by duck hunters in Louisiana, No. 6 lead, to the most comparable of readily available steel loads, No. 4 steel, in an actual duck hunting situation. Results of this study showed that participating duck hunters hit significantly fewer ducks per shot with No. 4 steel shot than with No. 6 lead shot. Also, the proportion of ducks crippled per duck hit was significantly greater with No. 4 steel than with No. 6 lead.

The species composition of the bag was not significantly different between the two loads. Thus, no evidence was found to indicate that a higher proportion of any species was bagged by No. 4 steel or No. 6 lead.

The lead and steel loads tested did not show relative performance differences at different ranges. Both loads hit more ducks per shot at closer ranges than at longer ranges. The lack of a significant interaction between loads and distances in the ANOVA for ducks hit per shot implies no difference between relative effectiveness for the 2 distances categories. The nearly parallel lines (Fig. 2) indicate that the differences noted are consistent across the range of distances of shots in the study.

Similarly, the load by distance interaction for ducks crippled per hit was not significant. Both loads crippled more ducks per hit at longer distances than at shorter distances. Differences between loads were again consistent across all intermediate ranges (23-46 m).

The significant interaction term for ducks crippled per hit between blind and distance means that the number of ducks crippled per hit was higher at longer ranges for some blinds. This significant interaction may have been influenced by the size of the pond at each blind. Pond size was difficult to determine and rank due to interspersed vegetation and irregular shapes. However, the blind with the highest number of ducks crippled per hit was located on the smallest pond and the blind with the lowest number of ducks crippled per hit was located on the largest pond.

Hunting conditions encountered on Lacassine during this study were similar to conditions, in general, in the marshes of southwest Louisiana during the same years. Poor duck nesting success during 1980 and 1981 resulted in a lower-than-normal harvest in south Louisiana. Low water levels and mild weather also contributed to the small number of ducks bagged in the study.

We believe that the hunters participating in this study were reasonably representative of southwestern Louisiana duck hunters, although no data are available for documentation. Most Louisiana hunters have had little experience shooting steel shot. Yet, LNWR has required the use of steel shot for the past 8 years, so some test hunters had experience with steel. Lacassine, being an established public hunting area, has its own clientele of avid hunters who continued to use the area by participating in this study. In the other extreme, many novice hunters participated in the study, especially in 1981, when they did not need access to a boat capable of traversing 10 to 19 km (6 to 12 mi) of water to reach the blind.

The species composition of ducks bagged was reasonably representative of the area. The composition of the bag at Lacassine during the 2 years prior to the study (US Fish and Wildlife Service 1979, 1980) was similar to the bag during the test (Appendix A). Some of the difference is attributable to the slightly lower than normal number of mallards and higher number of shovelers bagged during the years of the test.

The effectiveness of a shotgun load can be conceptualized as the probability of hitting a duck (hit per shot) and the probability of recovering a duck that is hit (bagged per hit). The probability that a duck was hit with a particular shot was not precisely measured because the observers did not record if a duck was hit by more than 1 shot. However, ducks hit per shot is a reasonable index to the probability of hitting a duck.

The proportion of the ducks actually hit that were recovered (bagged per hit) was measured by its complement, ducks crippled per hit. This variable has not been used in previously published studies. Crippled per hit was chosen because it accurately measures the conditional probability of recovering a duck after it has been hit.

The variable, ducks crippled per shot (Anderson and Sanderson 1979, Humburg et al. 1982), is the product of ducks hit per shot and ducks crippled per hit. In the Lacassine study, ducks hit per shot was significantly less for No. 4 steel, while ducks crippled per hit was significantly greater. These conflicting tendencies may result in the variable ducks crippled per shot not being significant when used.

Neither variable, hit per shot or crippled per hit, attempts to measure the total number of ducks that are hit and never retrieved. These variables do, however, compare the relative effectiveness of the 2 loads tested.

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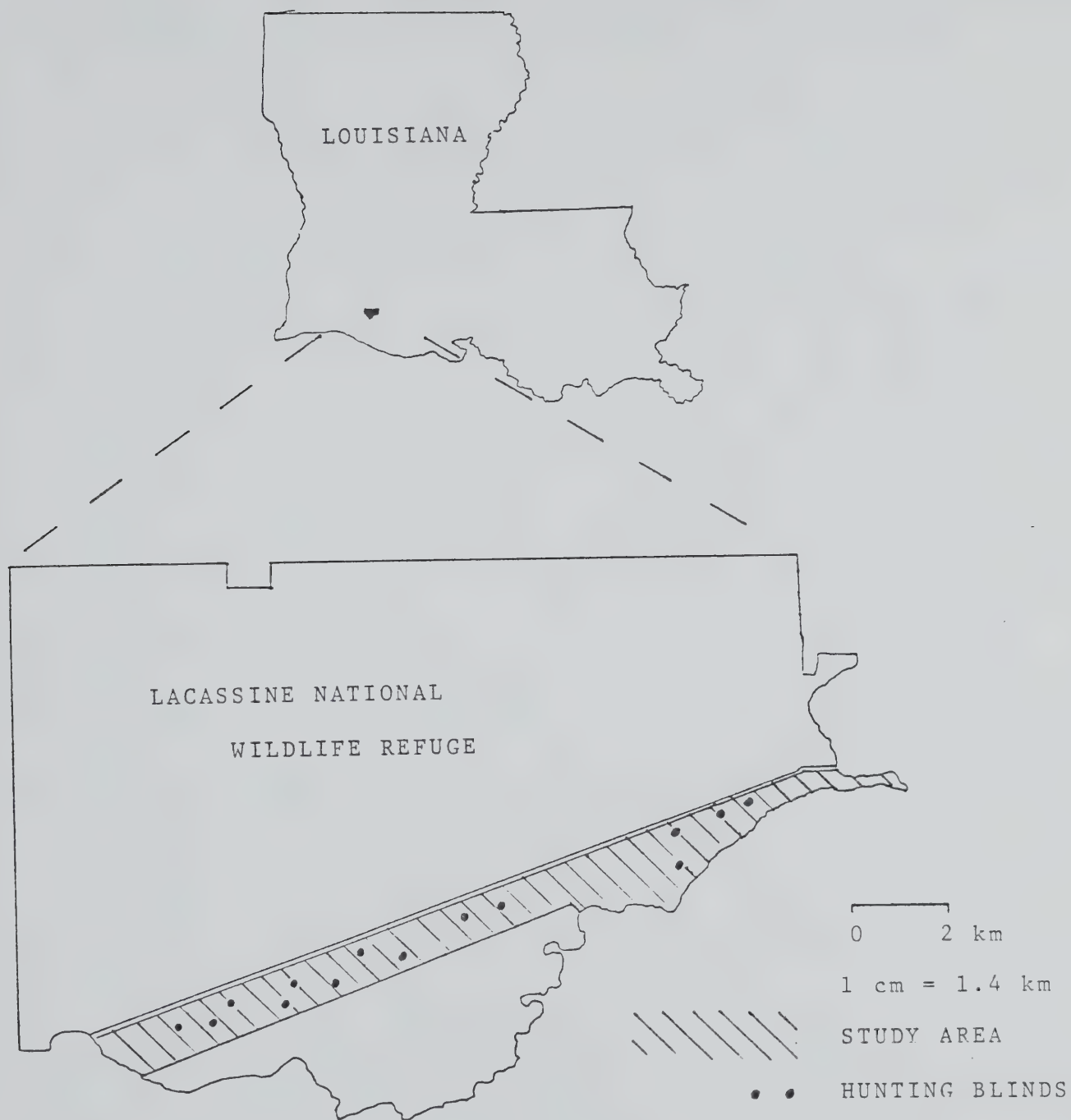
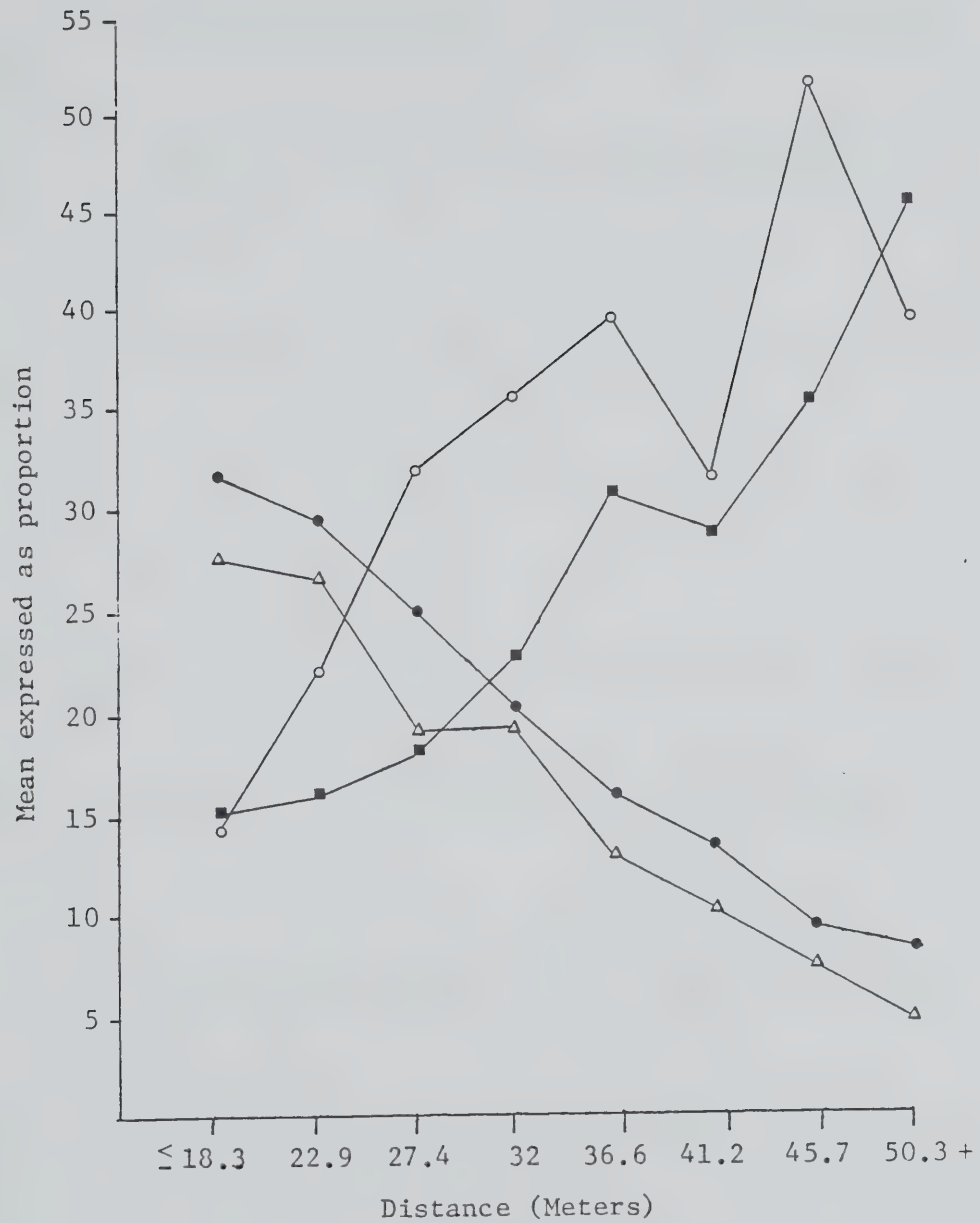


Figure 1. Location of study area, Lacassine National Wildlife Refuge, Louisiana.

Figure 2. Plot of proportion of ducks hit per shot and ducks crippled per hit at 4.6 meter (5 yd) distance intervals.



- Hit per shot, No. 6 lead
- △ Hit per shot, No. 4 steel
- Crippled per hit, No. 6 lead
- Crippled per hit, No. 4 steel

Table 1. Listing of 96 sampling units (12 blinds by 2 years by 2 loads by 2 distances) used in final analysis of the Lacassine study.

Blind No.	Days Used		Distance (meters)	Number Bagged		Number Crippled		Shots Fired	
	Lead	Steel		Lead	Steel	Lead	Steel	Lead	Steel
1980									
2	12	8	≤ 32	29	11	4	4	121	82
3	10	9	> 32	15	12	5	4	146	149
			≤ 32	27	22	6	8	112	105
6	8	12	> 32	8	8	12	6	95	110
			≤ 32	19	36	5	13	90	176
7	13	11	> 32	11	8	1	3	55	196
			≤ 32	58	43	15	9	279	243
14	7	11	> 32	29	9	12	2	181	141
			≤ 32	8	16	2	5	51	100
21	10	10	> 32	6	7	3	8	84	137
			≤ 32	19	11	4	4	71	42
22	14	13	> 32	10	3	2	3	78	69
			≤ 32	18	22	0	1	59	120
23	13	18	> 32	5	12	4	3	79	189
			≤ 32	37	24	1	6	131	126
25	11	14	> 32	16	15	6	15	162	260
			≤ 32	23	45	4	4	97	210
26	16	13	> 32	15	16	4	5	130	147
			≤ 32	28	20	6	14	150	123
27	15	13	> 32	23	6	9	4	181	154
			≤ 32	19	19	2	7	103	85
28	18	17	> 32	10	2	9	7	153	114
			≤ 32	59	30	10	13	253	228
			> 32	27	17	5	8	230	232

Continued.

Table 1. Continued.

Blind No.	Days Used		Distance (meters)	Number Bagged		Number Crippled		Shots Fired	
	Lead	Steel		Lead	Steel	Lead	Steel	Lead	Steel
1981									
2	17	20	≤ 32	34	36	10	10	161	235
3	21	18	> 32	11	6	11	5	193	143
			≤ 32	45	61	12	15	233	313
6	19	18	> 32	20	13	9	10	201	198
			≤ 32	86	60	27	30	408	375
7	15	18	> 32	27	22	12	7	242	194
			≤ 32	36	47	5	22	136	300
16	16	18	> 32	18	17	13	7	264	181
			≤ 32	41	28	9	18	187	231
22	16	19	> 32	10	11	13	9	174	229
			≤ 32	29	36	10	14	174	190
23	17	21	> 32	13	7	5	5	177	197
			≤ 32	31	38	8	13	175	213
24	18	14	> 32	17	17	4	13	179	259
			≤ 32	24	6	6	6	132	78
25	18	17	> 32	17	2	5	4	164	79
			≤ 32	47	23	13	13	204	171
26	18	20	> 32	16	18	10	12	191	289
			≤ 32	50	37	10	5	207	156
27	18	19	> 32	5	9	6	5	185	162
			≤ 32	37	26	6	16	174	225
28	20	17	> 32	10	3	7	7	151	124
			≤ 32	74	31	13	19	327	221
			> 32	25	18	11	15	293	314

Table 2. Comparison of ducks retrieved or lost when shots were fired at wounded birds on the water using No. 6 lead and No. 4 steel.

	<u>Retrieved</u>	<u>Not Retrieved</u>
No. 6 lead	291	42
No. 4 steel	251	52

Table 3. Frequency of responses of hunter parties when asked if they knew what load they were using.

Hunter parties response to which load was being used	<u>Load Actually Used</u>	
	No. 6 lead	No. 4 steel
Did not guess	224	216
One guessed lead	29	18
One guessed steel	33	51
Both guessed lead	25	10
Both guessed steel	38	61
One guessed lead, one guessed steel	10	11

Table 4. Number of blind-days when dogs were used or not used each year for each load.

	1980		1981	
	Lead 6	Steel 4	Lead 6	Steel 4
Dog used	23	41	34	36
No dog used	123	106	179	180

Table 5. Comparison of mean number of ducks hit per shot and crippled per hit by distance for hunter parties with and without dogs.

Was Dog Used?	Distance (meters)	Means [*]	
		Hit per Shot ¹	Crippled per Hit ²
No	≤ 32	0.245	0.231
Yes	≤ 32	0.250	0.183
No	> 32	0.115	0.377
Yes	> 32	0.124	0.290

^{*}Mean of all observations.

¹Mean calculated as total number of hits divided by total number of shots.

²Mean calculated as total number crippled divided by total number of hits.

Table 6. Number of ducks bagged, crippled and shots fired for each load, year and distance category.

Distance (meters)	Number bagged		Number crippled		Number Shots Fired	
	Lead 6	Steel 4	Lead 6	Steel 4	Lead 6	Steel 4
<u>1980</u>						
32	344	299	59	88	1517	1640
32	175	115	72	68	1574	1898
<u>1981</u>						
32	534	429	129	181	2518	2708
32	189	143	106	99	2414	2369
<u>Both Years Combined</u>						
32	878	728	188	269	4035	4348
32	364	258	178	167	3988	4267
<u>Total</u>						
	1242	986	366	436	8023	8615

Table 7. Species composition of bag for each year and load.

Species	Number of Ducks Bagged			
	1980		1981	
	Lead 6	Steel 4	Lead 6	Steel 4
Mallard	133	102	182	136
Mottled Duck	13	14	30	21
Pintail	52	41	41	37
Gadwall	82	64	118	90
Wigeon	20	11	57	37
Green-winged Teal	67	79	68	45
Blue-winged Teal	41	25	77	70
Scaup	4	3	24	14
Ring-necked Duck	31	30	36	52
Other ^a	85	71	107	90

^a Consisted mostly of Northern Shoveler and Wood Duck.

Table 8. The weighted analysis of variance for the arcsine of the square root of the hits per shot and crippled per hit in the Lacassine study.

Source	df	<u>Hit per Shot¹</u>		<u>Crippled per Hit²</u>	
		MS	F	MS	F
Blind	23	0.3654	-	0.6309	-
Load	1	8.165	37.44***	5.883	14.79***
Blind X Load (error a)	23	0.2181	.74	0.3978	1.78
Distance	1	106.089	360.47***	17.056	36.21***
Distance X Blind (error b)	23	0.2943	.79	0.4710	2.11*
Distance X Load	1	0.2754	.74	0.6608	2.96
Distance X Blind X Load (error c)	23	0.3722	-	0.2232	-

* P < .05.

*** P < 0.001.

¹Weighted by number of shots.

²Weighted by number of hits.

Table 9. Means for hit per shot and crippled per hit for each load and distance.

Distance (meters)	Means ^a	
	Hit Per Shot	Crippled Per Hit
<u>No. 6 lead</u>		
≤32	0.2630	0.1564
>32	0.1372	0.3264
All distances	0.1963	0.2360
<u>No. 4 steel</u>		
≤32	0.2326	0.2632
>32	0.0963	0.4095
All distances	0.1585	0.3340

^aBack-transformed means.

Appendix A. Comparison of species composition of ducks bagged during the Lacassine study and 2 previous years.

Species	Percent of Total Bag			
	1978-79 ^a	1979-80 ^b	1980-81 ^c	1981-82 ^c
Mallard	26	35	24	24
Mottled Duck	5	3	3	4
Pintail	6	8	10	6
Gadwall	15	13	15	16
Wigeon	8	6	3	7
Green-winged Teal	16	16	15	8
Blue-winged Teal	9	8	7	11
Scaup	-	-	1	3
Ring-necked Duck	5	5	6	7
Wood Duck	2	1	3	4
Other ^d	7	5	13	10

^a From US Fish and Wildlife Service 1979.

^b From US Fish and Wildlife Service 1980.

^c From Lacassine shooting study.

^d Primarily Northern Shovelers.

DISTRICT OFFICE
P.O. BOX 585
OPELOUSAS, LOUISIANA 70570

June 9, 1980

MEMORANDUM

TO: Bob Smith, Jacque Wiener, Jr., John Newsom, Ken Black,
Ray Aycock, Milton Friend

FROM: Larry Soileau *LS*

SUBJECT: Shotgun Shell Survey

I have attached a copy of Louisiana's recently completed shotgun shell usage survey. A total of 4,230 copies of the enclosed questionnaire was sent to a random sample of Louisiana duck hunters selected from recent state and federal waterfowl harvest surveys.

An individual record card was punched and verified for each completed questionnaire. No effort was made to purge loads which were not commercially available from the report with an edit program. We chose instead to report all responses exactly as they were received.

If you have any questions concerning this report, please give me a call. All completed questionnaires and return envelopes have been stored and are available for examination.

LS:ms

LOUISIANA DEPARTMENT OF WILDLIFE AND FISHERIES

Game Division

SHOTGUN SHELL SURVEY

May, 1980

LA. DEPT. OF WILDLIFE AND FISHERIES
GAME DIVISION
SHOTGUN SHELL SURVEY
MAY 1980

GAUGE OF SHOTGUN SHELL USED FOR DUCK HUNTING

10 GAUGE	3	0.1%	3-INCH 20 GAUGE	47	2.2%
3-INCH 12 GAUGE	191	8.9%	2-3/4 INCH 20 GAUGE	125	5.9%
2-3/4 INCH 12 GAUGE	1512	70.8%	OTHER	7	0.3%
16 GAUGE	181	8.5%	UNKNOWN	70	3.3%

SHELLS MOST OFTEN USED FOR DUCK HUNTING

FEDERAL DUCK + PHEASANT	365	17.1%	WINCHESTER DUCK + PHEASANT	81	3.8%
FEDERAL HI-POWER	192	9.0%	WINCHESTER SUPER X	451	21.1%
FEDERAL PREMIUM	8	0.4%	WINCHESTER SUPER DOUBLE X	53	2.5%
REMINGTON DUCK + PHEASANT	151	7.1%	RELOADS	185	8.7%
REMINGTON EXPRESS	376	17.6%	OTHER	23	1.1%
REMINGTON NITRO MAG	23	1.1%	UNKNOWN	228	10.7%

SHOT USED IN THE SHELLS FOR DUCK HUNTING

7/8 OZ	39	1.8%	1-5/8 OZ	92	4.3%
1 OZ	122	5.7%	1-7/8 OZ	106	5.0%
1-1/8 OZ	349	16.3%	2 OZ	14	0.7%
1-3/16 OZ	16	0.7%	2-1/4 OZ	23	1.1%
1-1/4 OZ	923	43.2%	OTHER	12	0.6%
1-3/8 OZ	110	5.1%	UNKNOWN	198	9.3%
1-1/2 OZ	132	6.2%			

SHOT SIZE USED FOR DUCK HUNTING

2	7	0.3%	7-1/2	231	10.8%
4	293	13.7%	OTHER	5	0.2%
5	113	5.3%	UNKNOWN	126	5.9%
6	1361	63.7%			

TOTAL MAILING 4230
TOTAL RESPONSES 2136
RESPONSE RATE 50%

LA. DEPT. OF WILDLIFE AND FISHERIES
GAME DIVISION
SHOTGUN SHELL SURVEY
MAY 1980

GAUGE OF SHOTGUN SHELL USED FOR DUCK HUNTING

10 GAUGE	0	0. %	3-INCH 20 GAUGE	0	0. %
3-INCH 12 GAUGE	191	100.0%	2-3/4 INCH 20 GAUGE	0	0. %
2-3/4 INCH 12 GAUGE	0	0. %	OTHER	0	0. %
16 GAUGE	0	0. %	UNKNOWN	0	0. %

SHELLS MOST OFTEN USED FOR DUCK HUNTING

FEDERAL DUCK + PHEASANT	9	4.7%	WINCHESTER DUCK + PHEASANT	5	2.6%
FEDERAL HI-POWER	17	8.9%	WINCHESTER SUPER X	59	30.9%
FEDERAL PREMIUM	6	3.1%	WINCHESTER SUPER DOUBLE X	25	13.1%
REMINGTON DUCK + PHEASANT	5	2.6%	RELOADS	15	7.9%
REMINGTON EXPRESS	33	17.3%	OTHER	0	0. %
REMINGTON NITRO MAG	7	3.7%	UNKNOWN	10	5.2%

SHOT USED IN THE SHELLS FOR DUCK HUNTING

7/8 OZ	5	2.6%	1-5/8 OZ	47	24.6%
1 OZ	2	1.0%	1-7/8 OZ	45	23.6%
1-1/8 OZ	14	7.3%	2 OZ	5	2.6%
1-3/16 OZ	3	1.6%	2-1/4 OZ	2	1.0%
1-1/4 OZ	21	11.0%	OTHER	3	1.6%
1-3/8 OZ	17	8.9%	UNKNOWN	17	8.9%
1-1/2 OZ	10	5.2%			

SHOT SIZE USED FOR DUCK HUNTING

2	1	0.5%	7-1/2	10	5.2%
4	56	29.3%	OTHER	0	0. %
5	15	7.9%	UNKNOWN	8	4.2%
6	101	52.9%			

TOTAL MAILING 4230
TOTAL RESPONSES 191
RESPONSE RATE 4%

LA. DEPT. OF WILDLIFE AND FISHERIES
GAME DIVISION
SHOTGUN SHELL SURVEY
MAY 1980

GAUGE OF SHOTGUN SHELL USED FOR DUCK HUNTING

10 GAUGE	0	0. %	3-INCH 20 GAUGE	0	0. %
3-INCH 12 GAUGE	0	0. %	2-3/4 INCH 20 GAUGE	0	0. %
2-3/4 INCH 12 GAUGE	1512	100.0%	OTHER	0	0. %
16 GAUGE	0	0. %	UNKNOWN	0	0. %

SHELLS MOST OFTEN USED FOR DUCK HUNTING

FEDERAL DUCK + PHEASANT	271	17.9%	WINCHESTER DUCK + PHEASANT	70	4.6%
FEDERAL HI-POWER	139	9.2%	WINCHESTER SUPER X	320	21.2%
FEDERAL PREMIUM	2	0.1%	WINCHESTER SUPER DOUBLE X	21	1.4%
REMINGTON DUCK + PHEASANT	117	7.7%	RELOADS	143	9.5%
REMINGTON EXPRESS	268	17.7%	OTHER	13	0.9%
REMINGTON NITRO MAG	12	0.8%	UNKNOWN	136	9.0%

SHOT USED IN THE SHELLS FOR DUCK HUNTING

7/8 OZ	21	1.4%	1-5/8 OZ	38	2.5%
1 OZ	41	2.7%	1-7/8 OZ	51	3.4%
1-1/8 OZ	190	12.6%	2 OZ	7	0.5%
1-3/16 OZ	9	0.6%	2-1/4 OZ	11	0.7%
1-1/4 OZ	845	55.9%	OTHER	6	0.4%
1-3/8 OZ	80	5.3%	UNKNOWN	106	7.0%
1-1/2 OZ	107	7.1%			

SHOT SIZE USED FOR DUCK HUNTING

2	5	0.3%	7-1/2	156	10.3%
4	190	12.6%	OTHER	1	0.1%
5	79	5.2%	UNKNOWN	65	4.3%
6	1016	67.2%			

TOTAL MAILING 4230
TOTAL RESPONSES 1512
RESPONSE RATE 35%

LA. DEPT. OF WILDLIFE AND FISHERIES
GAME DIVISION
SHOTGUN SHELL SURVEY
MAY 1980

GAUGE OF SHOTGUN SHELL USED FOR DUCK HUNTING

10 GAUGE	0	0. %	3-INCH 20 GAUGE	0	0. %
3-INCH 12 GAUGE	0	0. %	2-3/4 INCH 20 GAUGE	0	0. %
2-3/4 INCH 12 GAUGE	0	0. %	OTHER	0	0. %
16 GAUGE	181	100.0%	UNKNOWN	0	0. %

SHELLS MOST OFTEN USED FOR DUCK HUNTING

FEDERAL DUCK + PHEASANT	41	22.7%	WINCHESTER DUCK + PHEASANT	3	1.7%
FEDERAL HI-POWER	18	9.9%	WINCHESTER SUPER X	36	19.9%
FEDERAL PREMIUM	0	0. %	WINCHESTER SUPER DOUBLE X	2	1.1%
REMINGTON DUCK + PHEASANT	13	7.2%	RELOADS	8	4.4%
REMINGTON EXPRESS	32	17.7%	OTHER	6	3.3%
REMINGTON NITRO MAG	0	0. %	UNKNOWN	22	12.2%

SHOT USED IN THE SHELLS FOR DUCK HUNTING

7/8 OZ	5	2.8%	1-5/8 OZ	2	1.1%
1 OZ	5	2.8%	1-7/8 OZ	3	1.7%
1-1/8 OZ	109	60.2%	2 OZ	1	0.6%
1-3/16 OZ	3	1.7%	2-1/4 OZ	6	3.3%
1-1/4 OZ	20	11.0%	OTHER	0	0. %
1-3/8 OZ	6	3.3%	UNKNOWN	15	8.3%
1-1/2 OZ	6	3.3%			

SHOT SIZE USED FOR DUCK HUNTING

2	0	0. %	7-1/2	30	16.6%
4	26	14.4%	OTHER	1	0.6%
5	10	5.5%	UNKNOWN	8	4.4%
6	106	58.6%			

TOTAL MAILING 4230
TOTAL RESPONSES 181
RESPONSE RATE 4%

LA. DEPT. OF WILDLIFE AND FISHERIES
GAME DIVISION
SHOTGUN SHELL SURVEY
MAY 1980

GAUGE OF SHOTGUN SHELL USED FOR DUCK HUNTING

10 GAUGE	0	0. %	3-INCH 20 GAUGE	47	100.0%
3-INCH 12 GAUGE	0	0. %	2-3/4 INCH 20 GAUGE	0	0. %
2-3/4 INCH 12 GAUGE	0	0. %	OTHER	0	0. %
16 GAUGE	0	0. %	UNKNOWN	0	0. %

SHELLS MOST OFTEN USED FOR DUCK HUNTING

FEDERAL DUCK + PHEASANT	9	19.1%	WINCHESTER DUCK + PHEASANT	0	0. %
FEDERAL HI-POWER	8	17.0%	WINCHESTER SUPER X	9	19.1%
FEDERAL PREMIUM	0	0. %	WINCHESTER SUPER DOUBLE X	1	2.1%
REMINGTON DUCK + PHEASANT	3	6.4%	RELOADS	3	6.4%
REMINGTON EXPRESS	9	19.1%	OTHER	0	0. %
REMINGTON NITRO MAG	3	6.4%	UNKNOWN	2	4.3%

SHOT USED IN THE SHELLS FOR DUCK HUNTING

7/8 OZ	0	0. %	1-5/8 OZ	1	2.1%
1 OZ	9	19.1%	1-7/8 OZ	2	4.3%
1-1/8 OZ	8	17.0%	2 OZ	0	0. %
1-3/16 OZ	1	2.1%	2-1/4 OZ	0	0. %
1-1/4 OZ	18	38.3%	OTHER	0	0. %
1-3/8 OZ	3	6.4%	UNKNOWN	4	8.5%
1-1/2 OZ	1	2.1%			

SHOT SIZE USED FOR DUCK HUNTING

2	0	0. %	7-1/2	8	17.0%
4	8	17.0%	OTHER	0	0. %
5	0	0. %	UNKNOWN	0	0. %
6	31	66.0%			

TOTAL MAILING 4230
TOTAL RESPONSES 47
RESPONSE RATE 1%

LA. DEPT. OF WILDLIFE AND FISHERIES
GAME DIVISION
SHOTGUN SHELL SURVEY
MAY 1980

GAUGE OF SHOTGUN SHELL USED FOR DUCK HUNTING

10 GAUGE	0	0. %	3-INCH 20 GAUGE	0	0. %
3-INCH 12 GAUGE	0	0. %	2-3/4 INCH 20 GAUGE	125	100.0%
2-3/4 INCH 12 GAUGE	0	0. %	OTHER	0	0. %
16 GAUGE	0	0. %	UNKNOWN	0	0. %

SHELLS MOST OFTEN USED FOR DUCK HUNTING

FEDERAL DUCK + PHEASANT	28	22.4%	WINCHESTER DUCK + PHEASANT	3	2.4%
FEDERAL HI-POWER	8	6.4%	WINCHESTER SUPER X	21	16.8%
FEDERAL PREMIUM	0	0. %	WINCHESTER SUPER DOUBLE X	3	2.4%
REMINGTON DUCK + PHEASANT	10	8.0%	RELOADS	11	8.8%
REMINGTON EXPRESS	24	19.2%	OTHER	3	2.4%
REMINGTON NITRO MAG	1	0.8%	UNKNOWN	13	10.4%

SHOT USED IN THE SHELLS FOR DUCK HUNTING

7/8 OZ	6	4.8%	1-5/8 OZ	1	0.8%
1 OZ	64	51.2%	1-7/8 OZ	4	3.2%
1-1/8 OZ	17	13.6%	2 OZ	0	0. %
1-3/16 OZ	0	0. %	2-1/4 OZ	2	1.6%
1-1/4 OZ	6	4.8%	OTHER	1	0.8%
1-3/8 OZ	2	1.6%	UNKNOWN	18	14.4%
1-1/2 OZ	4	3.2%			

SHOT SIZE USED FOR DUCK HUNTING

2	0	0. %	7-1/2	21	16.8%
4	10	8.0%	OTHER	2	1.6%
5	4	3.2%	UNKNOWN	6	4.8%
6	82	65.6%			

TOTAL MAILING	4230
TOTAL RESPONSES	125
RESPONSE RATE	2%

NORTH CAROLINA STATE UNIVERSITY | AT RALEIGH

INSTITUTE OF STATISTICS

RALEIGH DIVISION
Box 5457 ZIP 27607

24 October 1980

Mr. Larry D. Soileau
 Department of Wildlife and Fisheries
 P. O. Box 585
 Opelousas, Louisiana 70570

Dear Larry:

This is a report on the statistical characteristics of the experimental test of steel vs. lead shot to be carried out in Louisiana this fall, with my conclusions on the power. First I will describe the results (Results) of my work on this problem; this section is self-contained. Then, second, I will go into the background and show how these results were obtained (Explanations).

Results. Table I shows the results of the analysis of power of this test, stated as proportions that can be distinguished from some arbitrarily chosen proportion, here approximately the results with lead shot (labeled Comparison Value). One-tailed tests are assumed, with the direction of the test differing among the parameters.

Table I. Power analysis for Louisiana test of steel vs. lead shot; value that can be discriminated from the stated comparison value, with Type I error 0.05 and Type II error 0.20, using one-tailed tests and the arcsin transformation.

	Parameter				
	Bagged per Shot with interaction	Bagged per Shot without interaction	Crippled per Shot	Crippled per Hit*	Crippled per Bagged
Comparison Value	< 0.200	< 0.200	> 0.0500	> 0.200	> 0.250
Level of Data					
High	0.163	0.179	0.0621	0.260	0.351
Medium	0.159	0.172	0.0665	0.281	0.391
Low	0.155	0.166	0.0706	0.301	0.431

* Ducks hit = ducks bagged plus ducks crippled.

With number bagged per shot we are only interested in whether use of steel shot resulted in fewer bagged than with lead shot. With number crippled per shot we are only interested in whether using steel shot results in more cripples per shot. With cripples per bird hit, we are again interested only in whether using steel shot cripples more than using lead shot. I believe that it is reasonable to use a one-tailed test for each of these questions. For each parameter, a separate value is calculated for each level of data. These levels of data are specified in Table II; that termed "medium" assumes a total of 10,000 shots fired and 2,000 ducks bagged.

In Table I, the two columns under bagged per shot represent separate calculations, one allowing for an interaction of load and blinds, with the other assuming no such interaction. Surprisingly enough, such an interaction appeared in the Missouri study and I see no way of predicting whether it will appear in the Louisiana study or not. There is more discussion of this interaction below. The two righthand columns in the table simply state the same values for crippling in a different way, first as cripples per bird hit and second as cripples per bird bagged; it is more convenient to carry out these statistical calculations with the first quantity but the second is probably more understandable by the hunter. The two are related by the expression $b = h/(1-h)$ where b is cripples per bagged and h is cripples per bird hit.

As an example of use of the table, if the medium level of data is achieved the difference that can be discriminated in birds crippled per bird bagged would be between lead shot at 0.250 and steel shot at 0.391, an increase of 56 percent. The test would do better with the high level of data available and less well with the low level.

If you wish to use comparison values that depart much from those given here (which are approximately the results from the Missouri study) then the values that can be discriminated should be recalculated because the actual differences vary depending upon the position in the proportional scale from 0 to 1; this fact is illustrated by the figures given here for crippled per shot as compared to bagged per shot (without interaction) where the tests are about the same power.

Explanations. These numbers in Table I have been derived through use of tables in J. Cohen (1969. Statistical Power Analysis for the Behavioral Sciences. Academic Press). The tables used (pp. 27-37) are for the t test and are appropriate here because we have only two shell types (one steel, one lead) even though the analysis of variance is used to derive the error variance and make the actual test. To select which table to use, one must state Type I error (I used 0.05), Type II error (I used 0.20) and whether the test is to be two-tailed or one-tailed (I chose the last).

Next, to use the chosen table, one must know the sample sizes involved and the error standard deviation. Given the sample sizes, the table give a value that Cohen calls the "Effect Size Index"; this is multiplied by the error standard deviation to estimate the difference detectable. Thus, beyond the statistical characteristics that are specified, one must know the sample sizes and the standard deviation.

The sample sizes follow from the experimental plan. There will be 20 blinds and at each blind each shell type will be used in a random pattern. Thus, considering that there will be 3 distance categories, there will be 60 measurements made for each shell type. While the numbers of shots fired and numbers of ducks bagged and crippled are also truly elements of sample size, in terms of this discussion, and use of the Cohen tables, these numbers do not contribute to sample size. Rather, as subsamples they determine the magnitude of the standard deviation; the greater these numbers, the less the standard deviation, and the more powerful the test.

When using the arcsin transformation in the form: arcsin of the square root of the proportion, and expressing it in radians, the average value of the error variance will be $1/4n$ where n is the harmonic mean of the numerical denominators of the various proportions. Such a relationship holds if each basic unit of the study is based on a single homogeneous proportion (not a mixture of proportions). In the usual experience we expect the actual error variance to be greater than this theoretical value because a mixture of proportions may be expected. With the Missouri study, however, this formula predicted the error variances quite well when the data were summarized in 12 different ways (3 parameters, each summarized according to 4 different bases). But for the Louisiana study the harmonic mean is unknown, although the arithmetic mean number of shots or of birds crippled plus bagged is known, subject to your assumptions of total numbers of shots, ducks bagged, and ducks crippled, as presented in Table II, and the fact that there will be 120 units of data in the analysis (20 blinds, 2 loads, and 3 distances). Therefore, I calculated the linear regression of the logarithm of the error variance on the logarithm of the arithmetic mean for the 12 different ways of working up the data from the Missouri test. This relationship can be expressed as follows:

$$y = 1.175867 - 1.428527x$$

where:

y = natural logarithm of error variance

x = natural logarithm of arithmetic mean number of the denominator

The error variance referred to here is the so-called "measurement error" or $B \times L \times D$ in the analysis of variance plan (Table IV).

This relationship was used with the expected mean numbers of shots fired and of ducks bagged and crippled (Table II) to estimate the variances to be expected without

Table II. Anticipated characteristics of Louisiana test of steel vs. lead shot.

Level of data	Shots	Total numbers of		Number per unit, of	
		Ducks bagged	Ducks crippled	Shots	Ducks bagged plus crippled
High	15,000	3,000	750	125.0	31.2
Medium	10,000	2,000	500	83.3	20.8
Low	7,500	1,500	375	62.5	15.6

interaction (Table III). For cases where interaction was accounted, these values were increased by the variance component calculated for the interaction of blinds and loads in the Missouri study, multiplied by the coefficient 3. The standard deviation to be used in estimating the discriminating ability (Table I) is the square root of the corresponding value in Table III.

Table III. Error (B x L x D) variances predicted for the Louisiana test of steel vs. lead shot, based on the anticipated characteristics as shown in Table II, and use of arcsin transformation (radians).

Level of data	Error variance (B x L x D) expected for:		
	Bagged per Shot	Crippled per Shot	Crippled per Duck Hit*
High	0.003275	0.003275	0.02378
Medium	0.005848	0.005848	0.04244
Low	0.008815	0.008815	0.06401

*Ducks hit = ducks bagged plus ducks crippled.

I assume that the basic experimental unit will be the results (shots fired, birds bagged and birds crippled over the whole season at each blind for each shell type; for each unit the following ratios will be calculated from these sums: bagged ^{at each distance} per shot, crippled per shot, and crippled per bird hit (hit = bagged plus crippled). Then the analysis of variance will proceed with these proportions, arcsin transformed. Probably a weighted analysis should be used because the proportions are based on varying sample sizes; this discussion assumes an unweighted analysis.

For the analysis, the model in Table IV is appropriate. It seems to me that this experiment conforms to a "split block" design rather than a "split plot" because distance is determined by the event; trials are not assigned at random to distances.

Table IV. Form of analysis of variance, and expected mean squares, for Louisiana test of steel vs. lead shot.

Source of Variation	Degrees of Freedom	Expected mean square
Blinds	19	$\sigma^2 + 3\sigma_{LB}^2 + 2\sigma_{DB}^2 + 6\sigma_B^2$
Loads	1	$\sigma^2 + 3\sigma_{LB}^2 + 60\theta_L^2$
L x B	19	$\sigma^2 + 3\sigma_{LB}^2$
Distance	2	$\sigma^2 + 2\sigma_{DB}^2 + 40\theta_D^2$
D x B	38	$\sigma^2 + 2\sigma_{DB}^2$
L x D	2	$\sigma^2 + 20\theta_{LD}^2$
L x D x B	38	σ^2

Here σ^2 designates a variance component for a random effect and θ^2 that for a fixed effect, with the effect indicated by the subscript used. Blinds are assumed to be selected at random from some large population; loads and distances are fixed.

This layout of the analysis of variance shows that the appropriate error to test Loads is the term Load x Blind; if this term is about the same size as the measurement error (Load x Distance x Blind), one may assume no interaction of load with blind and use a pooled error term. It still seems to me that an interaction of load and blind should not be expected, but with bagged per shot in the Missouri study, this interaction was significant ($0.025 < p < 0.05$) when using blinds as the blocking element (and highly significant ($p < 0.005$) when using blindsets, the blocking element actually planned in that study). The interaction was not statistically significant in the analysis of data from either crippled per shot or crippled per bird hit.

I spent considerable time examining this interaction but I do not yet really understand it, except that the data show that for certain blinds (or blindsets) the comparative results with the different loads differ from those for other blinds. For this reason, I calculated the power of the Louisiana test under the two conditions of whether there will or will not be an interaction of loads and blinds.

Sincerely yours,



Don W. Hayne
Professor

cc: Mr. Joseph Colson
Dr. Vernon Wright

Appendix D. Number of ducks bagged and crippled and number of shots fired of each load in 4.6 meter (5 yards) intervals.

Distance (meters)	Number Bagged		Number Crippled		Number Shots	
	Lead 6	Steel 4	Lead 6	Steel 4	Lead 6	Steel 4
≤ 20.1	201	183	36	31	725	756
21.0-24.3	226	208	44	59	889	980
25.6-29.3	262	181	55	88	1240	1395
30.2-33.8	226	181	68	104	1408	1450
34.2-38.4	155	105	72	71	1404	1343
39.3-43.0	103	77	43	37	1056	1070
43.9-47.5	39	25	22	28	624	707
≥ 48.5	30	26	26	18	677	914

Appendix E. Data for blinds that were excluded from analysis due to insufficient number of hits (bagged plus crippled less than 50) in 1980.

Blind	Distance (meters)	No. 6 lead			No. 4 steel		
		Ducks Bagged	Ducks Crippled	Shots Fired	Ducks Bagged	Ducks Crippled	Shots Fired
11	≤ 32	14	2	49	5	2	40
11	> 32	6	2	52	1	3	23
12	≤ 32	0	0	0	11	0	25
12	> 32	0	0	0	1	0	6
13	≤ 32	6	0	30	8	0	25
13	> 32	3	2	34	3	2	41
15	≤ 32	3	0	5	1	0	7
15	> 32	0	0	2	0	0	6
16	≤ 32	5	3	47	7	1	24
16	> 32	3	0	25	0	1	48
17	≤ 32	2	2	20	1	0	7
17	> 32	2	1	25	1	0	9
18	≤ 32	3	0	16	1	0	4
18	> 32	3	1	33	1	0	15
19	≤ 32	11	4	56	9	0	33
19	> 32	5	5	60	1	4	55
20	≤ 32	0	0	0	1	1	10
20	> 32	0	0	15	1	0	6
24	≤ 32	6	2	28	5	1	19
24	> 32	7	6	109	1	4	44
29	≤ 32	1	0	1	22	1	64
29	> 32	0	0	27	10	5	87

Appendix F. Summary of choke types used by hunters in the Lacassine study, by load type.

Chokes Used ^a	Number of Times Used	
	Lead 6	Steel 4
Full	223	240
Modified	335	332
Open	18	22

^aGuns having more than one choke were omitted from this table.

State Natural History Survey Division

ENR



Natural Resources Building
607 East Peabody Drive
Champaign, IL 61820
217/333-6880

Illinois Department of
Energy and Natural Resources

9 August 1982

Mr. Gaylord Donnelley
2223 Martin Luther King Drive
Chicago, Illinois 60616

Dear Mr. Donnelley:

Frank Bellrose told me that you sent him a copy of the Lacussine Steel Shot Report and asked for his comments on it. Bill Anderson, Biologist with the Illinois Department of Conservation who used to work for the Natural History Survey and is still housed with us, gave me a copy of his comments on the Lacussine Report and a copy of his comments on Tom Roster's review of the Report.

I thought that you might be interested in these comments and am taking the liberty of enclosing copies for you. It seems to me that the main problems with the Lacussine Report are that the authors apparently did not report all of the results they calculated and that they reported only ducks hit and crippled, with the problems summarized in the middle of page 8 of Roster's memorandum to Alan Wentz.

Sincerely,

Glen C. Sancerson, Head
Section of Wildlife Research

GCS:cj
Enclosures

cc: Dr. F.C. Bellrose



Illinois
Department of
Conservation
life and land together

office memorandum

to: T. Miller and Dennis Thornburg

from: Bill Anderson

date: 5 August 1982

subject: Comments on Lacussine Steel Shot Report
(and on Roster's review of same)

Attached is a copy of Tom Roster's review of the report entitled "Relative Effectiveness of No. 4 Steel and No. 6 Lead Shot for Hunting Ducks--the Lacussine Study."

Most of Roster's comments, all of which may be technically valid, do not cause me much concern. However, his criticism of the authors for expressing crippling rates relative to birds hit, and not expressing crippling relative to shots fired (as was done in most previous tests), has merit. The authors attempted to measure only the relative effectiveness of the two test shells for producing (or not producing) cripples. Unfortunately, crippling is influenced by at least three additional variables:

(A)		(B)		(C)		(D)		(E)
Hunters		Hunters		Local		Performance		
Hunting	+	Shooting	+	Hunting	+	of	=	Crippling
Skills		Skills		Conditions		Shells		Rate

Under actual hunting conditions, it is impossible to measure the performance of shot shells without taking the other variables into consideration. In the case of the Lacussine study, hunters shooting skills (Variable B) differed between the lead shot and steel shot--i.e., fewer birds were hit with steel. The crippling rates for steel and for lead differed, in part, because hunters were less skillful at shooting steel than at shooting lead.

My major criticism of the Lacussine report is the authors did not interpret their findings relative to the biological impact on the waterfowl population--i.e., how many more (if any) cripples would be left in the marsh if steel were used. In this regard, crippling rate should be expressed relative to hunting effort, as for example cripples per shell fired, cripples per blind-day, or cripples per hunter-day. The latter is probably the best (it eliminates concern about whether shooting opportunity or bag limit determines the number of birds shot), but data for making the necessary calculations were not included in the report. I have therefore used the data in Table 6 to calculate crippling relative to shells fired:

Lacussine - Ducks

Distance	Cripples Per 100 Shells	
	Lead 6	Steel 4
<35 yards	4.7	6.2
>35 yards	4.5	3.9
All yardages	4.6	5.1

Note that at distances >35 yards the crippling rate was 12.3% less for steel than for lead. For all yardages, the crippling rate was 11.0% greater for steel; this difference was not statistically significant when subjected to the t test.

For our own testing on Canada geese in southern Illinois, crippling rates expressed via the Lacussine method, via the methods we used in our reports, and relative to hunter-days are tabulated below.

Illinois - Geese

2 3/4-in 12 Gauge (1977)	Lead 2	Lead BB	Steel 1	Steel BB
Per 100 geese hit ^a	47.6	43.8	45.9	36.5
Per 100 geese bagged ^b	90.9	78.0	85.0	57.6
Per 100 shells fired ^b	12.9	12.5	13.2	8.8
Per 100 hunter-days ^c	71.4	74.4	61.8	41.3
3-in 12 Gauge (1978)	Lead 2		Steel 1	Steel BB
Per 100 geese hit ^a	26.4		23.2	21.6
Per 100 geese bagged ^b	35.8		30.5	27.4
Per 100 shells fired ^b	7.0		5.4	5.7
Per 100 hunter-days ^c	46.3		36.0	35.3

^aLacussine method.

^bMethods used in our reports.

^cProbably best method for measuring impact on population.

Note that all methods produced essentially the same results--i.e., crippling with steel was about the same as or was less than the crippling with lead. Also note that cripples per hunter-day was greater for the lead shells than for the steel shells in both years. In other words, the lead shells had a greater negative impact on the population (i.e., left more cripples in the field) than did the steel shells.

The bottom line to all this is: how crippling is expressed depends on the objective of the study. If testing effectiveness of shot shells per se is the goal, perhaps the Lacussine method is appropriate. However, if the biological impact on the population is to be determined (and this is really what counts), crippling should be expressed relative to hunting effort.

CC: V. Wright
R. Kasul



NATIONAL WILDLIFE FEDERATION

MEMORANDUM

Dr. W. Alan Wentz

Date: June 30, 1982

Tom Roster

Lacassine shooting test draft report

To expedite this series of comments, I've circled in blue and labeled the subject passages on the enclosed copy of the Lacassine Draft Final Report.

Comment 1: The authors display in the title, but more importantly in the abstract, a serious misunderstanding of what they actually tested at Lacassine. The authors use language which indicates they feel they tested No. 4 steel vs. No. 6 lead shot. This in fact is a gross overgeneralization.

The manner in which a projectile or projectile type is loaded plays just as much of a role in the projectile's performance as the nature, size, and shape of the projectile itself. This is particularly true of shotshells, where the weight of the shot charge, wad column, hardness of the lead pellets, and their velocity level play a profound role in the performance of the lead pellets used regardless of their size. The same can be said of steel pellets. Therefore, it is impossible in a shotshell test to isolate shot size as the variable you tested, unless you test all possible ways of loading that shot size. And you control all load variables except shot size. The manner in which shotshells are loaded, therefore, works in an interdependent, synergistic relationship with the shot type and size loaded. You can't isolate out one from the other in a field test such as Lacassine. In fact in a field test such as Lacassine load confounds shot type and size.

Example: If you compared a 3" 20 ga. 1 oz. load of soft lead 6's traveling 1200 fps vs. a 3" 20 ga. 1 oz. load of steel 4's traveling at 1350 fps would you get the same performance on ducks from steel 4's vs. lead 6's that the Lacassine authors recorded for lead 6's and steel 4's when loaded in the specific 12 ga. versions tested at Lacassine? Would you get the same performance if you tested a 1/100 oz. load of lead 6's vs. a 1-1/8 oz. load of steel 4's?

Another example: If you conducted a steel vs. lead test on elephants, and the lead load consisted of a .22 caliber 40 gr slug vs. a 400 gr steel slug in a .45 caliber load, both traveling at the same speed, and if the steel slug outperformed the lead slug, would you be justified in concluding steel outperforms lead for killing elephants?

Because load always works in conjunction with shot type and size in shotshells, and can't be separated out in a field test such as Lacassine, researchers should be very precise in their understanding of what treatments they actually tested, and what the results mean in general for the one treatment type vs. the other treatment type. All that was tested at Lacassine was one version of one size, of one treatment type vs. one version of one size of another treatment type on a specific target type (ducks) under specific field conditions when used by an untrained hunter population.

In terms of the Lacassine test, then, the relative effectiveness of a Federal 2-3/4" 12 ga. 1-1/8 oz. load of No. 4 steel was tested against a Federal 2-3/4" 12 ga. 1 1/4 oz. load of No. 6 lead on ducks in a heavily vegetated, wetland environment by an untrained hunter population which was supposed to have remained ignorant about which load type it was shooting.

Comment 2: Page 4, description of study area, second paragraph. A.) The fact that poor hunting conditions existed in Part I of the test vs. Part II affected the type of hunter that participated in the test. While in Part I there initially was high participation in the test, as the opportunity to bag ducks degenerated in the study area, hunters abandoned the study to seek better hunting opportunity elsewhere. The authors fail in their report to acknowledge this. Some hunters hung on throughout the test. This is the principal reason insufficient data were collected in year one of the study (Lacassine was designed to run only one year) and had to be continued a second year. The authors do not adequately acknowledge this fact. In year one, hunters had to use their own boats and skills to find their way several miles through bayous and frequently fog, to the study area. In year two, the hunters were ferried to the study area, plus the number of blinds were cut nearly in half. (See page 6, methods discussion). Had I been consulted I would have argued strongly against ferrying hunters in year two. My experience with such services is that you do not get the same kind of hunter population as when you require them to supply their own boats and skill to get to the site. You probably get a less skilled hunter in a ferry operation. At any rate ferrying was inappropriate in year two because a ferry was not used in year one and all variables should be kept the same as possible from year one to year two. At Lacassine NWR the tradition had been for years for hunters to supply their own equipment and skills to get to hunting sites. By instituting the ferry service, that tradition was radically altered, and surely affected the nature of the participants. The authors do not adequately address this problem.

It is questionable, therefore, how representative the hunters were that participated in the Lacassine test, especially year two, of the hunters that normally hunt the refuge. Scientifically speaking, the Part II data should be treated differently from the Part I data, since two serious changes occurred in test procedure, thus introducing both a variable and a bias. The authors seem to have glossed over this problem and lumped year one with year two data.

B.) The authors discuss briefly the difficulty the bottom composition and dense cover vegetation made retrieval of downed ducks at Lacassine. This is the first test condition that makes Lacassine unique from previous tests where-in retrievability was much less problematic. Retrievability can seriously affect whether a downed duck ultimately is classified as a cripple or as a bagged duck. The extreme retrievability problems at Lacassine could seriously alter, obscure, and affect definitional load performance. In other words, many ducks that were unretrieved at Lacassine would have been much more retrievable at other test sites, and would alter bagging and crippling totals.

Whether or not these conditions affected one load more adversely than another is not resolvable. Since it is not, crippling losses would be most fairly analyzed and compared on a per-blind basis. Lumping all blind site data

together (each blind site presented different degrees of retrievability problems) would tend to obscure this bias. The authors tried to clear it up by dividing data into dog used or dog not used (because a dog would increase retrievability), but this is not adequate.

Comment 3: Page 6, additional hunter assignment procedure, middle paragraph. 20% of the hunters providing data in the Lacassine test were not assigned blind sites by random. This represents another departure from previous test procedures where 100% of participants received blind sites by a system of randomization. This represents another bias in the Lacassine data and another uncontrolled variable.

Comment 4: Page 7, observer training discussion paragraph at top of page. While the observers at Lacassine were trained in the same manner, achieved as a group a similar level of pre-test skill in measuring ranges of hunter shots, and in determining evidence for a visibly hit but not retrieved bird as observer groups in previous tests, another serious departure from previous test procedures exists at Lacassine. The Lacassine authors failed to run follow-up tests of observer competence during the test and after the test to determine if range measuring, data recording, and cripple determination skills had fallen off, and if so to what extent. The other purpose of such tests was to weed out any observer that had fallen below minimum skill levels. The authors also failed to analyze and report pre-test observer skill levels.

Since all data in the Lacassine test was observer gathered and reported, the competence of the observers is vital to accurate data gathering. Since no analysis of pre-test observer skill data was conducted nor reported, and since no follow-up testing of observer competence was carried out, nor weeding out of observers who had lost sufficient skills, the accuracy of the Lacassine data is subject to question.

It is writer's experience (writer trained all observers in all observer steel vs. lead tests) that the skills used by the observers are learned skills and that such skills fade at different rates among different individuals, and that usually one or more observers have to be dropped from data gathering. It is a very real possibility that some or much of the data at Lacassine was inaccurately gathered.

Therefore, an unmeasurable inaccuracy bias exists in the Lacassine data that further clouds the results.

Comment 5: Page 7, last two sentences of shell details discussion paragraph. The velocity levels and pellet counts for the lead and steel loads reported by the authors are merely taken-on-faith averages as reported to the authors by Federal Cartridge Corporation. The authors chose to have no independent ballistic laboratory check the velocity and pressure levels of the test ammunition. This is another difference in the Lacassine test from previous tests. In previous tests confirming laboratory tests were run on test ammunition and both factory claimed and actual test velocities were reported. Since factory ammunition is loaded to nominal velocity level specifications which allow a latitude of + or - 50 fps from the norm, it is not unusual to find factory ammunition velocities varying substantially from what the factory may claim

for the ammunition. In addition no antimonial level information was provided for the test lead shot ammunition. This is vital to knowing the nature of the lead beast being tested.

More seriously, the Lacassine authors departed still again from previous test procedures by opting to forego necropsies of one-shot kill ducks. This procedure was carried out in two of the three previous most comparable tests. In addition no on-test site pattern testing, nor any pattern testing of any kind was conducted on test ammunition at Lacassine, while such testing was conducted in all three previous tests. Necropsy, velocity, and pattern testing data are vital to understanding the actual performance values of the test ammunition. More importantly, such data are the only means by which an understanding of why results happened can be gained. At Lacassine what happened will be known; an insight as to why will never be possible.


Failure to gather these kind of data causes more reason for doubt on Lacassine results as the possibility exists the ammunition tested was a.) substandard in some way, b.) one or more of the test loads possessed anomolytic performance characteristics and c.) the manner in which the test ammunition patterned through the modified chokes which predominated at Lacassine in the atmospheric conditions which existed at Lacassine may have resulted in important differences in the hunter's opportunity to hit with one load vs. the other at the shooting ranges that predominated in the Lacassine test. In other words, how the loads performed in terms of velocity and patterns at Lacassine may have caused hitting to play a more important role in ammunition performance than in other tests.

Comment 6: Page 7, last paragraph, discussion of who remained ignorant of load type. Authors mention that only hunters and observers remained ignorant of loads. It is noted that administrators are not included. If administrators had knowledge of load number or load type during the test and during analysis, a breach in protocol occurred at Lacassine. Writer developed a very detailed protocol for the Service for Lacassine which insured hunters, observers, and administrators would remain ignorant of number of loads tested, load type, and load details, until after all data had been analyzed. [REDACTED]
[REDACTED] Such knowledge on the part of any author can insert serious uncontrolled bias into the data analysis and report itself.

Page 8, middle of first paragraph. By the fact that administrators of the test are again excluded from the list of who remained ignorant of load type details, it may again be inferred that administrators and perhaps one or more authors may have been privy to some load information which observers, hunters, and on-site test personnel were not privy to. If so, this is a major departure from the three previous tests. It is unclear the degree of knowledge of load type and number the statisticians possessed while analyzing the data and writing report results.

Page 2, bottom - the statement that hunters and observers did not know whether lead or steel was used in their blind each day is an inaccurate statement for the hunters. This statement is contradicted by post-hunt survey results detailed in paragraph two, page 12 of the report. The authors report that of the

39% of the hunters who indicated knowledge of load type while participating that hunt day, they were correct in their identification of load type to a degree far greater than chance. Therefore, a significant portion of 39% of the data collected at Lacassine was gathered from hunters knowledgeable about load type. These data were not treated separately by the authors, but lumped with data collected from hunters from whom there is no indication they knew what they were shooting.



Comment 7: Page 7 and 8 confusion-load discussion. The distribution of confusion-loads on only a 5% basis is not frequent enough to effect any meaningful confusion in the participants.

Comment 7: (Last paragraph, page 8) At Lacassine shots fired at ducks for each attempt were subdivided on the data cards into total shots at ducks, and number of shots fired at cripples on the water. It is unclear throughout the test report whether any data analysis involving shots fired was based on the total shots fired or just the shots fired at ducks in the air (total shots fired - number of shots fired at cripples on water). Whether total shots are used or just shots in air can have a significant impact on the outcome of any analysis using or related to shots fired. In previous steel vs. lead tests total shots were used in any shots fired related analyses.

Comment 8: (Page 9, middle of last paragraph) The authors report calculating ducks hit per shot and ducks crippled per hit. It was also reported to writer in personal communication with two authors that cripples per shots fired and cripples per blind day were also calculated. From these data no significant differences could be detected for lead or steel in either analysis.

Cripples per shots fired is the analysis used to report crippling in three previous tests. If it was calculated at Lacassine why was it not reported? If it wasn't calculated at Lacassine it should have been to allow comparability among the tests. If the authors calculated crippling by any analysis besides cripples per hit, they are seriously remiss in not reporting the results of such calculations. The reason is that just which analysis is used is largely a matter of opinion since crippling is a conceptual and definitional entity in these tests not an actual, verifiable fact.

In all scientific reports, objectivity is best served by reporting the results of all analyses and allowing the reader to determine the value of each analysis. Authors should not be in the business of playing editor and based upon opinion selecting the analysis or analyses they wish to report.

In all previous tests all methods of analyzing cripples derived by the authors were reported by the authors. If the Lacassine authors did indeed analyze crippling by several methods, but elected only to report the cripples per hit analysis

then this represents another departure, and a quite serious one in terms of objectivity, in the Lacassine report from the ^{previous} steel vs. lead test reports.

Comment 9: End of second paragraph, page 11, under results. Conspicuous by its absence in the Lacassine report is the mean distance of shots taken in the test. This was reported in all previous tests. By working through this paragraph it becomes obvious that the mean distance of shots at Lacassine was about 30 yards. The 35-yard breakpoint in distance increments throughout the report is actually related to cripple totals rather than shot distances. Here is another difference between the Lacassine report and other tests where distance increments were based on the distance of shots fired, not cripples or bagged birds.

More importantly, the fact that the mean distance of shots fired at Lacassine was only 30 yards, makes Lacassine a test of a specific steel load of No. 4's vs. a specific lead load of No. 6's at close ranges. The mean distance of shots fired at Lacassine is closer than in any other steel vs. lead test. It is closer than Schell-Osage by 6.5 yards, closer than Illinois by 16 yards, and closer than Tule Lake by 20.5 yards. The extremely close mean distance of shots fired at Lacassine makes it on this basis alone a different test of shotshell performance than any other comparable steel vs. lead test.

Comment 10: Page 12, second paragraph and part of third paragraph. Here the authors decide to include data that could be confounded with the effects of the load. Not only does this generally enter bias into the results but one very disturbing statistic is glossed over by the writers. They report that in the post-hunt survey 39% of the hunting parties indicated they knew what they were shooting, and of these the hunters correctly identified load on a scale greater than would be expected by chance. This means a significant portion (the authors never stipulated the actual number or percentage) of 39% of the hunting parties in the Lacassine test knew what they were shooting, and thus the double-blind nature of the experiment was not operational for this group.

Yet, because a very few repeat hunters were incapable of correctly differentiating loads, the authors conclude that the 39% group as a whole also could not! This is preposterous and not supported by their own data. See Comment 6.

Lumping of the data from the 39% group with the hunters who remained ignorant is not justifiable, as the purpose at Lacassine as in other tests was to test ammunition performance in a hunter population that did not know what it was shooting.

[REDACTED]

[REDACTED]

Comment 11: Page 12, bottom paragraph. The authors report 57% of the hunters used modified chokes and only 40% used full chokes. This is another difference of Lacassine vs. the other tests, where 80% or more of the hunters in the other

tests used full chokes. Choke can significantly affect hunters' ability to hit ducks or geese when shooting lead or steel.

Comment 12: Page 13, end of first paragraph. Authors discover or report that retrieving dogs can significantly affect the retrievability of crippled ducks in environments such as Lacassine. Since whether or not a downed duck is retrieved or not affects whether or not it is classified as a bagged or crippled duck, this in turn affects any crippling analysis.

Comment 13: Page 13, first paragraph under comparison of loads. From the data the authors decided to use for analysis purposes (some was thrown out) it can be determined that Lacassine hunters fired 16,638 shells to bag 2,228 ducks. That works out to about one duck bagged for every 7.5 shells. Hunters also crippled a total of 802 ducks, or about 1 cripple for every 21 shots fired.

In comparison, Schell-Osage hunters firing at essentially the same target species at a mean distance 6.5 yards further away, bagged one duck for every 5.3 shots fired and crippled one duck for every 22 shots fired. Lacassine hunters were significantly less efficient in hitting ducks than Missouri hunters, or in other words, were poorer shots as a group than Missouri hunters.

At Schell-Osage, Illinois and Tule Lake, hunters hit the target species almost equally well on a per shots fired basis with either the lead or steel loads tested. At Lacassine the hunters hit (bagged plus crippled) 1608 ducks with 8023 lead shells and only 1422 ducks with 8615 steel shells. The Lacassine hunters, unlike the hunters in all the other tests, had more trouble hitting ducks with the steel load tested than the lead load. This is confirmed by the authors comments in the first paragraph on page 14 and page 15 of the draft report. This is still another area in which the Lacassine test differs from the other tests.

The importance of this fact cannot be overlooked. Because the authors chose to analyze and report crippling on a hit dependent basis (cripples per hit), if there is a disparity between load types in the hunters ability to hit with one load vs. the other, this shooting ability will significantly impact the resulting crippling values. In other words, at Lacassine shooting skill impacted load performance more than in any other tests because a.) the Lacassine hunters had more difficulty hitting with the steel load than the lead load while in other tests they hit nearly the same with both load types, and b.) the authors chose to use a hit-dependent or shooting skill related analysis method for looking at crippling, so that whatever load the hunters had the most difficulty hitting with, the resulting cripple ratio will be the highest for that load.

The variable cripples per hit is not a clean variable for comparing load performance because it is muddled by shooter skill in using the loads and discriminates against the load the hunters have the most difficulty shooting.

Comment 14: Page 17, second paragraph. It is irrelevant if a duck is hit by each shot. What counts is how many ducks were hit for all the shots fired, or the proportion of ducks hit per shot fired for one load vs. another.

Comment 15: Page 17, third paragraph. The authors point out that the variable or analysis they used for reporting cripples (cripples per hit) was different than for any of the previous tests. This is true and the principal reason the Lacassine test is different from the other studies.

The authors then try to justify that cripples per hit is the best indicator of crippling losses (or at least why they chose it over cripples per shot, or cripples per bagged or cripples per blind day). Their justification is very much subject to disagreement and there is a great deal of opinion not fact in their justification. As previously discussed, the big problem with any analysis based on hitting is that hunter shooter skill is now dragged into the variable. Cripples per hit is very dependent upon how many ducks the shooter was able to hit with a load. This is a very muddy way to analyze crippling or bagging performance of shotshell loads. Remember, we want to learn how the loads of steel vs. the loads of lead bag or cripple ducks. If you make bagging or crippling dependent upon how many ducks the hunters were able to hit with each load, then you make your analysis of shell performance less sensitive, and more dependent upon shooter skill, or more the result of a shell/shooter interrelationship.

Example: Lacassine hunters fire 10 shots of lead and 10 shots of steel. With the lead load they bag 6 ducks and cripple 4. With the steel load they miss with 2 shots, bag 4 ducks and cripple 4 ducks.

	<u>Lead</u>	<u>Steel</u>
<u>Shots Fired</u>	10	10
Misses	0	2
Ducks Bagged	6	4
Ducks Crippled	4	4

Now the actual number of cripples for each load is identical. But, because the hunters hit less ducks with the steel load, the percentage of cripples produced by the steel load is higher than for the lead load. So, on this analysis the steel load crippled a higher percentage of ducks than the lead load, or of the ducks struck (hit and crippled) the steel load crippled a higher percentage. This is exactly what the cripples per hit analysis does and this is exactly the way the authors reported the Lacassine shooting test results. And because cripples per hit is so dependent upon shooter skill (it's the shooter's skill that largely determines his hits, not the load) it is a very misleading indicator of crippling losses.

Cripples per shot, while not perfect is not as dependent upon shooter skill as is cripples per hit, and thus is the best means of comparing losses of one load vs. another and the best indicator of real world crippling losses.

Summary: The Lacassine test under close examination is more unlike than like the previous steel vs. lead tests. This is so because of some procedural differences, some environmental differences which adversely affected retrievability of downed ducks and thus the crippled vs. bagged totals, some important analysis variations, the relatively close distance of shots taken, the number of loads tested, the chokes which predominated, and the apparent lower shooting skill level of Lacassine hunters in general and with the steel load in particular.

Of concern is that the Lacassine authors failed to test observers for declining measuring and recording skills, failed to test ammunition for velocity level and patterning performance, and failed to report crippling on the same basis as used in other tests. Of further concern is that data from hunters who correctly identified load type was lumped with data from hunters who could not identify load type. This introduces bias. Also of concern is that the sole crippling analysis reported is highly dependent on hitting success (which in turn interrelates with shooting skill) and thus automatically inflates the crippling rate percentage for whatever load the hunters were least able to hit with. That is why at Lacassine while the actual lead crippling loss total was 366 ducks and with steel, 436 ducks which is not significantly different on a per shot basis, the steel crippling total become significant and 41% greater than the lead total on a cripples per hit basis. This makes load performance highly dependent on the shooting skill level of a given hunter population. Results would vary, therefore, if the same loads were tested by a hunter population possessing more or less shooting skill.

Left in question is whether on any analysis involving shots fired, the authors used only shots fired at ducks in the air, or total shots fired for each attempt.

Recommendation: Lacassine data should be analyzed and reported on a cripples per shot basis in addition to the author's preferred cripples per hit basis. Data for both analyses should be reanalyzed after data from hunters who correctly identified load type is separated from hunters who could not identify load type. The authors should acknowledge in their discussion that the percentage of cripples per hit for either load are not good indicators of either the magnitude or comparative degree of crippling losses that will occur in the real world if these two loads are used. The authors should further acknowledge that the environmental conditions, extremely close mean shooting distances, choke preferences, and shooting skill level of Lacassine hunters may not be representative of hunting conditions and hunters for the nation as a whole.

Enclosures

TR/jlr

